

incorporate both the PTAR effect and the emerging network (Fox) effect into the model, we did not for reasons stated in the response to EI.8.

We agree that using our model to predict the future effects of the repeal of PTAR must be done carefully. We know that the presence of emerging networks and the continued diversification of cable options has changed the market. While our attempts to incorporate cable penetration proved statistically insignificant, we do not deny that more analysis could be performed. Until more and better data are gathered and analyzed -- a task that no other party has attempted -- we contend our results provide the best analysis of the past effects of PTAR and the best prediction of the future effects of the repeal of PTAR.

c. Response to the Specific WW Criticisms

We now turn to the criticisms made by WW.

WW.1 "Both reports are preoccupied with stations' ability to attract audiences but (with one exception) make no attempt to translate these observations into measures of viewer welfare. None of the estimated models are deduced from economic models that could be applied to explain the behavior of the television industry. ... And granting the existence of some systematic relationship, it is still unclear how the LECG and EI models could assist the Commission in its pursuit of policies ...

Contrast this approach with the work of Noll, Peck and McGowan [1973] ...

Implicitly, LECG ... assumes that their measures of independent station performance -- NAB rates of return on sales and Arbitron rating points -- are positively correlated with social welfare." WW, p. 21-22.

We recognize and endorse the analytic insights that can be gained from structural models, for example those developed by Noll, Peck and McGowan (1973). We would have liked to have developed and estimated a comprehensive structural model for this proceeding. However, time and data constraints did not allow for this. Given the data limitations that we faced (discussed in **Section B.3.a** above), we developed the reduced form models presented in our report. The model given primary scrutiny in our report was the model of average independent station ratings in a market. We also estimated, but did not report, a set of models of aggregate independent station ratings. We report those results in **Appendix A** of this document.

In these models, we take station ratings as reflecting viewers' revealed preference for the programs offered by the independent stations. We assume that an increase in the number of TV households watching an independent station reflects an increased revealed preference for the programs of that station, relative to the alternatives. While we have formulated neither a model of consumer utility nor a model of broadcast costs, we still believe that consumer utility is positively correlated with ratings.²⁴ Their rhetoric aside, we believe that WW would agree with this interpretation. If WW feel there is a better empirical measure of consumer utility, they should make it explicit.

WW.2 "In no instance are we provided with estimates based on a sample of network affiliates. This makes drawing conclusions from the empirical estimates a hazardous undertaking." WW, p. 22.

²⁴ This specifically refers to the econometric analysis. However, we have developed some of these models in **Section IV.B**.

This statement is a canard. Our analysis focuses upon the performance of independent stations, pre- and post-PTAR. We analyze whether PTAR had a statistically measurable effect upon the ratings of independent stations. We use the pre-PTAR period as the control group for our comparison.

It is true that we could have also modeled the countervailing effect of PTAR on the network affiliates.²⁵ Indeed, we considered that analysis. However, we decided not to pursue it because of time limitations and because it was unnecessary.

WW.3 "A failing throughout LECG's report is the absence of any distinction among the different independent stations. ...

It is important to account for such differences because large established firms and small marginal firms have differential impacts on the performance of an industry, ... Some accounting for these differences, and the differential impact of PTAR, is necessary. We will show how various measures of a station's 'age' will be a powerful explanatory variable for the effects of PTAR." WW, p. 23.

²⁵ For example, for the access period, the average affiliated station ratings over all markets from 1966-1976 were the following: 18.09, 15.46, 15.78, 15.54, 16.10, 14.27, 13.47, 13.52, 13.46, 12.72, 11.97. For exposition, we underline the post-PTAR ratings.

Clearly, we see a countervailing impact of PTAR upon network affiliates. If we introduced those network impacts into our econometric analysis, our measures of the relative impact of PTAR upon independents would have been larger.

We document this issue further in Figure IV.1 of our original report.

The preoccupation of WW on individual stations throughout their comments is a red herring, aimed at distracting the Commission from its appropriate focus in this proceeding.

As discussed at length in **Section B.3.a** above, the focus of the Commission in this proceeding should be broadcast markets, not individual stations. One of the aims of PTAR was to foster more nurturing market environments for the UHF spectrum, hence the independent stations in that spectrum. PTAR will have been successful in that regard if it raises the average and aggregate ratings of independent stations.

The success or failure of individual stations tells us nothing about PTAR. There would have been successful and unsuccessful individual independent stations whether PTAR was enacted or not. Hence, WW's preoccupation with individual stations is misplaced and flawed, and their econometric models derived from this preoccupation are misspecified.

To clarify this point further, consider the following analogous policy situation. Suppose the Federal Reserve and/or the Treasury implement stimulative macro policies and the stock market responds favorably with a 5% increase in a variety of market indices. Assume that it is clear that these policies stimulated the increase in the stock market. In spite of this general increase, some stocks will rise and some will fall, due to firm-specific factors. While the Fed and/or the Treasury may have trouble picking the individual winners and losers, they will have no trouble identifying the original source of the general market increase.

Another analogous policy situation would be the following. Suppose the EPA decides to clean up a watershed that was quite polluted and restore it to recreational-use quality standards. This policy effort will raise property values generally along that watershed. However, it is not assured that all property values will rise. Given other site-specific factors, some property values may remain the same or even fall, despite the overall positive impact of the EPA policy action. The EPA may have difficulty picking the winners and losers in terms of increased property value. However, the overall positive impact of the EPA policy is clear.

In this proceeding, as in these analogies, we respectfully submit that the Commission should not be concerned with analyzing, modeling or predicting the differential impact of PTAR upon individual independent stations. That analysis requires much greater station-specific information than is publicly available; that analysis is irrelevant to the primary issue facing the Commission.

WW.4 "In a data set this large (31633 records²⁶), it is nearly impossible to avoid errors of some kind. However, there are systematic mistakes in this data set that cannot be overlooked. ... Literally dozens of stations were misclassified. ... (e.g., classifying WNAC-64 in Boston when it is located in Providence) .. [and] (KTVW is channel 33 in Phoenix, not channel 13 in Seattle). There are even Canadian stations in the data set ... , which are obviously not subject to ... PTAR." WW, p. 24.

Several of these claims, which we can explicitly address, are completely unfounded. While the call letters WNAC now belong to a Providence station,

²⁶ Incidentally, this estimate of our total number of records is incorrect.

through most of the time period studied, the call letters belonged to a Boston station. Similarly, the call letters KTVW belonged to channel 13 in Seattle-Tacoma during most of the years of this study. Admittedly, CBET is a Canadian Station, but this error appears only in one year out of 14. In general, we assumed the Arbitron data to be accurate and did not subject it to checking against other data sources. We did double check our data entry and submitted our data base to standard verification procedures, including but not limited to, analysis of outliers and the distributions of the variables.

We cannot assess the validity of their claims regarding "dozens of stations" without more information. As they correctly state, there is always some error in almost any data base. Econometric methods are designed to accommodate such error. Even if their criticisms of the LECG database were wholly founded, the error would be quite small. For example, suppose we assume considerably more errors than they claim to have found, say 10 dozen errors in the 38,000 (approximately) observations. That error rate would be about 0.3% of the observations.

WW.5 "Gaps in the data also present a problem. ... Data are missing for certain years for several stations." WW, p. 24.

As discussed in **Section B.3.a**, we gathered all data possible under the time constraints of this filing. We did not purposely exclude any data. If some data were missing, we had to work with that problem, which is common to most econometric analysis.

WW understand the difficulties of gathering primary data and performing empirical work under the time pressures of these proceedings. These proceedings would have been better served if, rather than criticizing LECG's data base, WW had assisted in enriching the data base with 1980s data.

WW.6 "Oddly enough, the LECG data set may suffer from being too large. We found that many of the stations included in its sample failed to meet Nielsen's minimum reporting standards. This shows up in the Arbitron ratings where we find that 14.3% of the time the independent stations receive a zero rating for the access period. ... Frequency distributions of ratings reveals a collection of independents clustered at the very bottom of the ratings range, with another collection registering ratings distribution more like network affiliates." WW, pp. 24-25.

We did not have the advantage of accessing Nielsen data for years other than 1993. Instead, to assemble a consistent time series, we used the publicly available Arbitron data.

We reviewed the Arbitron data and examined both the sample size and the distributional issues mentioned by WW. However, because we contend that the proper focus of this analysis is the average or aggregate of independent stations, we did not focus upon subsets of stations within each market. WW seem to suggest by this criticism that we should have dropped certain observations. We felt and continue to feel that it would be irregular to arbitrarily truncate the sample within each market in this way, since arbitrary truncation can produce biased econometric estimates.

WW.7 "LECG's statistical methods in deriving its results are rather unconventional. Especially noteworthy are its tendencies to:

- select years for the time series that occur sporadically, precluding any possibility that the dynamic structure of these market could be properly specified;
- truncate the sample of markets arbitrarily, making inferences to smaller markets suspect;
- report point estimates for regression coefficients only if they are statistically significant, making it impossible to confirm LECG's model estimates." WW, p. 25.

LECG procedures were conventional. As discussed in **Section B.3.a**, the dimensions of the data base were imposed upon us by the time frame of the proceeding. As any econometrician would do, we adapted standard methods to the available data.

We followed standard reporting procedures for our econometric results. To wit, we reported those variables that proved to be statistically significant, and we did not report preliminary regression results and/or hypothesis tests.²⁷ We reported standard regression statistics. Our reporting procedures are similar to those of most applied academic journals and consulting analyses. The confidence intervals can be derived from the statistics that we did report, which we do in **Appendix A**.

²⁷ Preliminary regression analyses and hypothesis tests were conducted and reviewed electronically, and final regression results were thereby developed. Only final regression results were committed to hard copy. These are standard procedures.

WW.8 "As a consequence of these failings, it is not possible to put any confidence in the predictions of the model for cities outside the top 30, for years beyond 1993, ... In particular, LECG never reports confidence intervals for its estimates for the impact of PTAR on independent stations. .." WW, pp. 25.

To our knowledge, the best and only data base that exists for this analysis is ours, which is stratified by the top 30 markets for a subset of years from 1966-1993 (as discussed in **Section B.3.a**). While we know that the data can be improved, until that time we contend that the best econometric estimates available are derived from that data.

That being said, we agree that extrapolation beyond the bounds of the existing data must be done carefully. However, it is incorrect to state that "it is not possible to put any confidence in the predictions of the model." Because many of the markets ranked 31-75 have grown by 1993 to be the size of many of the top 30 markets in Period 1, we feel that extrapolation beyond the top 30 markets is not problematic. However, we fully agree that the broadcast world has changed since Periods 1 and 2.

As a result, extrapolation beyond 1993 must be done carefully. We feel that we have done so.

We deal with the issue of confidence intervals in our response to WW.18.

WW.9 "By taking averages of all independent stations in a market, LECG precludes any insight as to why independent stations perform differently. By further removing affiliate stations from the sample,

we lose all hope of learning how PTAR affects stations' performance..." WW, p. 26.

We dismiss these criticisms in **Section B.3.a**, and in responses to WW.2 and WW.3.

WW.10 "LECG estimates the ratings relationship controlling for several different features of television markets: the percentage of TV households ... and a crude measure of consumer purchasing power (PCI*TVHH ..)." WW, p. 26.

As discussed in **Section B.3.a**, we controlled for all other factors for which we had the time to develop the appropriate data. We tested for the independent effects of income (PCI) and TV households (TVHH) and found that the product of the two performed better, eliminating collinearity.

The characterization of this measure as "crude" is inappropriate, particularly for two analysts that use the "age" of the individual station (see WW.15 below) as the measure of relative operational and managerial efficiencies of the alternative independent stations.

All econometricians must use certain proxies for underlying economic measures. Per capita income and numbers of consuming groups (here TVHH) are standard proxies, used fairly universally. They may suffer from modest measurement error. There is no econometric data series that does not. However, they are used in spite of the modest measurement error because their exclusion would generate even more serious specification bias.

WW.11 "It estimates this relationship using a linear regression despite the fact that the dependent variable is confined to the range between 0 and 1. A linear regression is a poor specification because it quickly goes out of the 0-1 range, resulting in biased coefficients and especially poor predictions when forecasting in the extreme portions of the sample range." WW, p. 27.

WW are incorrect to state that "a linear regression is a poor specification ..., resulting in biased coefficients" in this case. A correct statement is that a linear specification may be a poor specification .. resulting in biased coefficients. It is equally likely that the linear specification may be a good one, resulting in unbiased coefficients. It is up to the econometrician to assess a variety of alternative specifications and exploit those that are found to be statistically most appropriate.

In modeling a dependent variable that ranges between 0 and 1, we have a wide variety of alternative specifications including but not limited to linear, logit, probit, and gompit. Any one of these may be a good or bad specification. The quality of the specification depends upon the specific facts of the application.

We specified and estimated linear and logit forms of the model. We found the statistical results similar. Furthermore, the linear model was not required to analyze or predict outside of the 0-1 range. The estimated average station rating model does not predict outside of the range of 0.0 to 0.154²⁸ for the historical period and for the policy extrapolations. As a result, we found no

²⁸ The aggregate station ratings range was 0.00 to 0.344.

reason to believe that the linear estimates were biased or that the linear model was inappropriate.

Furthermore, the criticisms made by WW are most relevant when the model is estimated using data for individual TV households. The use of data for groups of TV households in a given market (i.e., our data) eliminates many of the criticisms of the linear specification.²⁹ The use of such data requires correction for heteroscedasticity, as indicated in footnote 24, page 67 of Appendix D of our report.

WW.12 "First of all, this specification does not introduce any intertemporal dependencies, a serious omission since it is very likely that current station ratings depend very heavily on past performance. While this could be captured with lagged variables, the construction and interpretation of these variables is awkward given the sporadic nature of the time series." WW, p. 27.

We agree that our specification makes introduction of intertemporal dependencies awkward. We tried some intertemporal specifications involving a lagged dependent variable to relate "current station ratings" to "past performance", as suggested by WW. The results were statistically insignificant, which we attributed, in the course of the analysis, to the sporadic nature of the data.

²⁹ See Maddala, Limited-Dependent and Qualitative Variables in Econometrics, Cambridge University Press, 1983, pp. 28-30.

However, we contend that the use of an intertemporal specification involving a lagged dependent variable (which seems to be the proposal of WW) would be an inferior specification here. Such specifications are most useful in a time series situation: when economic responses are slow or occur with a lag; when data are therefore serially correlated; and/or when the only purpose of the analysis is to predict future levels of the dependent variable. In this situation, period t is highly correlated with period t-1 and the inclusion of period t-1 as a regressor is highly significant in predicting period t.

However, such an intertemporal specification is much less useful when attempting to analyze the underlying causal determinants of the dependent variable. In such a case, explaining period t effects with period t-1 effects begs the question. We must still explain period t-1 effects. Furthermore, when economic responses are rapid, as with the ratings effects of PTAR, past period effects have less explanatory power.

We are faced with this latter situation. We must determine the structural causes of independent station ratings in a market characterized by rapid economic responses. Intertemporal models will prove less appropriate for that analysis than the models that we have employed.

WW.13 "Second, cross sectional variation is not likely to be captured by the PCI*TVHH variable. This variable would provide a reliable proxy for real income of TV household in a market only if the number of individuals per household does not vary over time and across markets. Also there is no indication that LECG deflated per capita income using regional deflators, despite the fact that the largest 50 metropolitan areas ranged from 47.2% above to 17.8% below the national income." WW, p. 27-28.

The first part of this criticism is truly a quibble, particularly for two analysts that use "age" as a proxy for the operating experience and efficiencies of an independent station. We tried to gather and use household income as our measure of income. However, we were told that the information were available at the ADI level only for Census years (1970, 1980, 1990). In its absence, PCI is a very reliable proxy for the needs of this analysis.

In terms of the second criticism, we agree in theory. We wanted to use regional deflators for the analysis and attempted to gather a complete consistent set. However, such a consistent set of deflators were unavailable.³⁰ We contend that the potential measurement error introduced is much less serious than the specification error that would result from excluding the variable. We contend that the potential measurement error introduced does not in any way affect our analysis or conclusions.

WW.14 "Finally, in another part of their report, LECG concludes that whether a station broadcasts on the UHF band is an important determinant of its ratings. Nevertheless, this variable is not included in their specification of the ratings relationship. When we introduce a dummy variable for stations that broadcast in the UHF range, we find that it has a significant, negative coefficient. We

³⁰ We approached the Bureau of Labor Statistics (BLS) and The Statistical Abstract of the United States to obtain such data. We were told that the BLS data allowed for deflation over time for a variety of cities but did not make cross sectional comparisons.

The Statistical Abstract data, cited by WW, do allow for some cross sectional comparisons. However, the first year such data are available is 1968, and our time series began in 1966. Furthermore, different cities are included in different quarters and in alternative years. We were told that the significant gaps would exist for a time-series cross-sectional set of deflators. As a result, we could not develop a consistent set of deflators over all years and markets.

also never see any account taken of the relative use of off-network vs. syndicated programming, which is a key empirical question facing this preceding." WW, p. 28.

As mentioned above in **Section B.3.a**, we initially introduced all variables into the equation, including the penetration of UHF% reception in the ADI. When UHF reception proved to be statistically insignificant, we correctly eliminated it. We also indicate in that section, that we had wanted to characterize the programming of the independent stations but were unable to, given the time constraints of the analysis.

We never introduced the UHF status of individual stations because we did not undertake an analysis of individual station performance, for reasons discussed above in **Section B.3.a** and WW.3.

WW.15 "To gain a better understanding of what LECG has actually modeled, we estimated the very same ratings equation as reported by LECG, but we did not average the independent stations' ratings within markets. As a result we use observations for each of the 346 stations represented in LECG's database. ...

We then depart from the LECG specification by introducing several variables. ... The estimated coefficients are much different from those in the LECG report. ... The PTAR dummy is not statistically significantly different from zero.

What this shows is that ... LECG has suppressed substantial differences across stations. ... It is also little wonder why they get such a good "fit" for their model using market-specific variables. **This model merely picks up differences across markets in terms of how favorable they are to independent broadcasting [emphasis added].**

Next, we apply this same basic model to access and prime time periods for the network affiliates. ...

In our attempt to differentiate among stations, we then introduce several new variables that measure the 'age' of a station. When applied to network affiliates, we find that .. the various measures of age are not statistically significantly different from zero.

The case of independent stations is completely different. ... [while] the coefficient on the PTAR dummy was not significantly different from zero.

The picture that emerges is one in which PTAR has differential effects on independent stations [sic]. Apparently, PTAR results in a windfall gain to established independent stations. ... " WW, pp. 28-31.

As we discuss in **Section 3.a** and response WW.3, we completely reject the analytic approach proposed by WW. We did not and contend that we should not analyze the performance of individual stations. Furthermore, if we had, we would have specified a much different model focusing upon a much richer array of station attributes than the "crude proxy" of "age" used by WW.

We specified our model to assess the impact of PTAR upon markets, using the pre-PTAR years as a control group. It is therefore completely inappropriate for WW to apply our model, or our data base as it now stands, to the analysis of individual stations. Using our model for their analysis, the hypothesized impact of PTAR is assumed constant for each and every station in all markets in a given year, whether that station was successful or not. Since some stations were winners and some were losers, there will be by presumption no relationship between PTAR and those stations' performance. As a result,

WW's model imposes the condition that PTAR will have no effect. It does not derive that conclusion.

In order to capture the effect of PTAR upon individual stations, it is necessary to formulate a model that hypothesizes the interaction of PTAR effects with a richer specification of station attributes. We did not do this, because we did not feel it was the appropriate focus of the analysis. WW have not done it, either with our model or with their own.

For all of these reasons, we reject all of their econometric conclusions and results.³¹

WW go on to complain that there is "little wonder why they [LECG] get such a good "fit" for their model using market-specific variables. **This model merely picks up differences across markets in terms of how favorable they are to independent broadcasting.**"

PRECISELY! As discussed above, our model is designed to analyze the determinants of independent station performance at the ADI market level and test the hypothesis that PTAR improved that performance, ceteris paribus. As

³¹ While WW complain that we have not provided sufficient descriptive material for our reported regressions, they have provided absolutely none. Therefore, it is impossible to critically review their econometric claims. For example, they claim (footnote 43) that they "run the Goldfeld-Quandt test for heteroscedasticity" ... and find evidence that "LECG's choice of regression weights may be incorrect."

The application of this test is discretionary and its power can be limited if the sample is not large enough. Without further information from WW, we cannot judge whether they have appropriately applied the test. Furthermore, their claim is curious since the weights used are those suggested by theory, as cited in footnote 24, page 67, Appendix D of our report.

we have argued in **Section B.3.a**, this is the correct measure of ratings impact. Furthermore, it is this measure that is most relevant for the analysis of market entry by independent stations.

Having identified the overall market effects of PTAR, we fully agree and explicitly state that some stations will be more successful than others. We develop measures of the relative success in 1987 of the independents that will become Fox affiliates. We documented the continued success of these stations in 1993.

WW.16 "The main purpose of LECG's rating equation is to predict the effect of repeal of PTAR ... In conducting this exercise, LECG makes several serious errors, any one of which invalidates their conclusions.

Monumental events have impacted the television industry since 1970 and PTAR has not had the greatest effect. ..." WW, pp. 31-32.

We agree that monumental changes have affected the television industry since 1970. As discussed above, we attempted to incorporate as many of those factors as possible in our specification and data.

Furthermore, we understand the care that must be taken with extrapolating econometric results beyond the sample period. As discussed in response to WW.8, we believe that our model estimates are sound and the best

statistical estimates available in this proceeding. They are logically, therefore, the best available statistical estimates for predicting the future.³²

WW.17 "Over the 1995-2004 period, the quadratic time trend alone contributes 2.5-4.62 points to independent ratings. ... We cannot calculate the contribution of the linear and cubic terms because their coefficients are not reported for either model, even though they are used to compute the cumulative PTAR effect." WW, p. 33.

WW mis-speak here. We used standard econometric procedures. To wit, we hypothesized an effect for all independent variables for which we had data. We estimated their effects. Following standard procedures, we eliminated those variables for which there was no statistically measurable effect.

In the equation referenced by WW, we found no statistically measurable effect for the linear and cubic terms. We dropped those variables from the reported equation and from any predictions made with the reported equation. These terms were not used to compute the cumulative PTAR effect.

WW.18 "Of course, these point estimates are not meaningful unless we include their confidence intervals, something that LECG does not produce." WW, p. 33.

³² Any standard econometric text develops the proof that best linear unbiased estimators provide best linear unbiased prediction.

We agree that confidence intervals would be useful. They are provided in Appendix A.

WW.19 "Furthermore, LECG derived its ratings reduction by taking the simple average over the top 30 markets. ... weighting the reductions by the number of TV households, we find that the reduction in ratings over the top 30 markets comes to -1.69 points, which is 28% less than the unweighted average of -2.34 points reported by LECG." WW, p. 34.

We estimate our ratings equation using data from 30 markets, the size of which is controlled for by our independent variables. When we simulate the effects of PTAR in the past, we do it for synthetic (Table D.5) or real markets (Table D.6). Likewise, when we predict future impacts of PTAR repeal, we do it for real markets.

In no case do we take averages over the 30 markets to simulate past effects and/or future effects. As a result, we do not understand the point being made here. Without further clarification we do not understand the correction being proposed.

WW.20 "Taking the coefficient estimates from our model using unaveraged ratings, ..." WW, p. 34.

Because we reject the WW model and analysis of individual stations, we reject these coefficient estimates. PTAR was designed to address structural

issues in the top 50 ADI markets, not to address the problems of some individual independent stations.

WW.21 "A more subtle error that LECG commits has to do with how it computes the effect of PTAR's repeal. LECG claims to answer the question: what change would occur in independent station ratings during the period 1995-2004 if PTAR was repealed in 1995? In fact, LECG answers the question: what ratings would independent stations earn if there had never been a PTAR?" WW, p. 34.

WW have mischaracterized our methods. The calculation they claim we have made is indeed the method by which we estimated the net impacts of PTAR over the historical period (1971-1993). Through this calculation, we correctly attempt to answer the question: "What ratings would independent stations earn if there had never been a PTAR?" The difference between these predicted ratings (absent PTAR) and those occurring with PTAR is our estimate of the net impact of PTAR in the past. (See Tables D.5, D.6 and footnote 17, p. 54, Appendix D of our report).

When we simulate the future repeal of PTAR, we do not make that assumption. The method we do use is described in footnote 22, p. 60, Appendix D of our report, where we set the PTAR variables to zero in 1995, as WW suggest.³³ In this simulation therefore we assume that PTAR-related effects were operative through 1994 and became zero in 1995.

³³ Their confusion is clear from their footnote 55, in which they refer incorrectly to our footnote 17. The methods we used for this calculation are described in footnote 22.

However, there is a subtle point of relevance made by WW here. That point is that our reported simulation of the 1995-2004 period after repeal of PTAR is the same whether PTAR existed or did not exist in the past.

As discussed in **Appendix A** below, when we made our forecast simulations, we felt that our reported method was better than several alternative methods considered. However, this point made by WW suggests that a supplemental forecast simulation may be appropriate. We describe such an alternative in **Appendix A**.

WW.22 "Finally, note the negative coefficient on the variable PTAR*PCI*TVHH for both the access period and for all three time slots. ... Perhaps this conclusion would change if LECG properly adjusted per capita income for differences across markets [sic] in the cost of living and the average size of households." WW., p. 35.

As discussed in response WW.13, this criticism is a quibble. We used the best proxy for income and market size available to us.

Furthermore, WW have ignored one of the iron laws of econometrics, which is that measurement error biases estimated results to zero. If our measure of income is measured with error as they claim, then use of a better proxy would reduce measurement error and increase the measured effect, rather than "change" it as they hypothesize.

WW.23 "This exercise is marked by the complete lack of any economic modelling. ... They make little effort to control for the role of expected profitability for independent stations." WW, p. 38.

We have fully described our economic model and its purposes in **Section B.3.a** and selected responses to WW. For those purposes, we do not need to control for expected profitability, even assuming we could get the necessary data.

We have argued that this model and its focus upon markets is appropriate for this proceeding. We have argued that the models used by WW to analyze individual station performance are wholly inappropriate to this proceeding and fundamentally flawed for the purposes to which they are put.

WW.24 "LECG recognizes the importance of the increases in cable penetration over these years for independent stations. But they do not include the variable %CAB as before because of difficulties in obtaining coefficient estimates. ...

It is really no wonder that cable penetration is not estimable: the penetration rate of cable closely follows a time trend that increases geometrically, .. Apparently the PTAR dummy did not have a significant effect so LECG called upon the time trends to control for this effect." WW, p. 39.

To more precisely reiterate our procedure, we included %CAB in our regression equations but could not estimate a measurable cable effect at statistically acceptable levels. The fact that we could not identify a cable effect may be due to multicollinearity, as WW hypothesize. However, we examined

the correlation matrix of all independent variables and did not find %CAB and %CAB² excessively correlated with the other variables, including the time trends. Hence, we do not think that collinearity caused its statistical lack of impact.

WW.25 "Perhaps the most glaring omission from LECG's entry equation is any account of the FCC's television station allocation plan" WW, p. 39.

We do not perceive our omission of the FCC's station allocation plan as "glaring". Instead, in our initial analysis, we perceived FCC allocation procedures as likely accommodating, rather than causing, requests made by prospective entrants as a result in part of the PTAR stimulus. These entry effects, stimulated by higher ratings at the market level, are expected by economic theory and by WW and EI (as evidenced by the comments cited in **Section B.2**).

Upon further conversations with counsel, we have become convinced that WW fundamentally misconceive the FCC allocation plan. Specifically, virtually all of the UHF allocations were established in the 1952 allocation plan. There was no acceleration of additional UHF allocations during the 1966-1993 time period. The considerable increase in UHF stations since 1975 demonstrates that new incentives induced entrepreneurs to begin television operations on previously allocated but unused UHF channels.

III. ABSENT PTAR, BROADCAST NETWORK TELEVISION IS NOT A COMPETITIVE MARKET IN 1995, BUT WITH PTAR IT IS MORE COMPETITIVE THAN IT WOULD BE OTHERWISE

A. AN INCREASE IN ADVERTISING DEMAND ONLY LEADS TO LONG RUN PRICE INCREASES WHERE MARKET POWER EXISTS, CONTRADICTING EI'S CLAIM OF A COMPETITIVE MARKET

In its reply comments, EI argues that "LECG fails to discuss the most plausible reason for an increase in network advertising rates -- increased demand for advertising -- and focuses instead on an implausible market-dominance argument."³⁴

The increase in network advertising prices that EI agrees did in fact occur happened in a sustained way throughout the 1980s, a fact pointed out also by former Disney President Richard Frank in the Finsyn proceedings.³⁵ We certainly agree with EI that an outward shift in the demand curve for advertising did occur during this long run period. However, as is clearly illustrated in **Figure III-1**, such an increase in demand does not lead to an increase in advertising rates if, as EI claims, the market at issue is competitive. Such an increase in demand would lead to increased entry and prices would remain relatively stable under competitive conditions.³⁶ But, in fact there are significant barriers to entry into this market that revolve largely, though not solely, around the scarcity of VHF spectrum and how that spectrum has been allocated and

³⁴ Economists, Inc. (1995), p. 39.

³⁵ Frank, Richard, Summary Statement of The Walt Disney Studios, before the Federal Communications Commission, MM Docket 90-162 En Banc Hearing, December 14, 1990.

³⁶ See P. Sraffa, (1925) "The Laws of Returns Under Competitive Conditions," Economic Journal, Vol. 36, pp. 535-550.